STOCKHOLM SCHOOL OF ECONOMICS
Department of Economics
659 Degree project in economics
Spring 2021

Military Conscription and Local Crime

Simon Norrman (24206) and Malcolm Thunberg (24201)

Abstract: The defence bill passed by the Swedish parliament in 2020 instigates a continued expansion of the Swedish Armed Forces and an increase in the number of military conscripts for the coming years. The bill includes an expansion of military conscription, a compulsory enlistment dominated by young men. As young men are most statistically prone to commit violent and property crime, this study sets out to analyse the connection between conscription and crime. Previous research has not found a conclusive connection between military service and future criminal convictions. However, this paper contributes to previous research by analysing the contemporary effect of Swedish conscripts on municipal crime between 1997 and 2008. The dataset forms a panel of 46 Swedish municipalities with at least one male conscript at any time of the studied period. Furthermore, we differentiate between normal municipalities and "base municipalities", i.e., regions with a high conscript-to-population ratio, to analyse if the conscript-effect is higher for some municipalities than others. Our findings indicate that the number of conscripts does not significantly affect regional crime levels, nor is a significant effect found for base municipalities. We conclude that the absence of effects may be due to an incapacitation effect, conscripts not being a representable sample for the population of young men and military culture deterring criminal behaviour.

Keywords: Crime, Conscription, Defence Spending, Demography

JEL: K42, H56, J10

Supervisor: Robert Östling
Date submitted: May 17, 2021
Date examined: May 25, 2021
Discussants: Jakob Norling and Nicolas Navia
Examiner: Johanna Wallenius

Table of Contents

List of Tables

List of Figures

1 Introduction

Although the Second World War ended nearly eight decades ago, young adults all over Europe face mandatory military training, or conscription service. In Sweden, conscription dates back to 1904 until it was abolished in 2009. However, it has recently been reintroduced in 2017, with more military bases planned to open in the next four years and an increase in the defence budget from 29 bn SEK in 2020 to 89 bn SEK in 2025 (TT 2020)

Since conscription engages a large portion of the young demographic, it is not surprisingly a topic of political debate. Proponents of conscription usually advocate its educational qualities and affirm that the military's strict and disciplined culture turn "boys into men". Furthermore, proponents often pertain that conscription is an important integration policy since it intermingles people from different socioeconomic and ethnic backgrounds. For example, Anders Lindborg (2015), editorial writer for one of Sweden's largest newspapers Aftonbladet, writes that "Some form of conscription service is important for integration and a sense of unity. It would present an opportunity for many to contribute to their homeland and be a direct way to become a part of the Swedish society."

While Lindborg and his adherents argue for a positive economic effect of conscription, empirical research conversely indicates that there may be indirect economic costs. For instance, Keller et al. (2016) find a negative causal effect on post-service tertiary education in OECD countries. Moreover, conscription has historically only applied to young men, therefore, consisting of a demographic prone to commit criminal offences. Military service's effect on crime has also been extensively researched although resulted in inconclusive findings (Baktir et al. 2020). Baktir et al. (2020) present two possible channels through which military service affects crime propensity: a negative causal effect is hypothesized to relate to the military's sense of unity, education, and job training while a positive causal effect is attributed to the military's violent and stressful environment. The discrepancy in the research on military service and crime is also true for Scandinavian studies. Hjalmarsson and Lindquist (2019) find a positive causal effect on post-service crime in a study on Swedish conscripts in the 1990s and early 2000s. However, during military service, they find a reduction in criminal activity due to an incapacitation effect. On the contrary, a longitudinal study of draft-lottery in Denmark concludes that military service decreases the propensity for young people to commit property crime and no effect on violent crimes (Albæk et al. 2017).

So, given the inconsistency in previous academic literature, what is the true cost of expanding military conscription as Sweden plan to do? Is there a large indirect cost not accounted for, in terms of an increased propensity to commit crime during conscription?

This paper sets out to examine just that. Building on previous research, we analyse whether conscription is a significant determinant for contemporary crime in Swedish municipalities. The dataset utilised in this paper combines data on the number of Swedish conscripts with the number of reported crimes for several crime categories between the years 1997 and 2008. The data forms a panel of 46 municipalities that had at least one conscript in 1997. The sample size is also incrementally reduced based on the municipalities' conscript concentration, to measure the effect in what we label as "base-municipalities".

2 Background

This section is intended to give a brief overview of Swedish military history and an introduction to the Swedish conscription system. The substantial reorganisation of the Swedish Armed Forces (SAF) stretching over the studied period is also described. The background is provided to give a meaningful context and to better understand the reason for the choice of empirical methodology in this paper. This section is mainly based on information provided by the SAF.

2.1 The Swedish setting

Following the Second World War, the SAF remained at a high capacity and enlisted around 50 000 conscripts annually. All Swedish men between the age of 18 and 47 were subject to conscription in 1958 and the high level of preparedness remained and even heightened until the middle of the 1960s. 1964 is often regarded as the pinnacle of the Swedish Armed Forces, it could mobilize around 800 000 soldiers and the Swedish air force was considered among the greatest in the world. However, this was followed by a steady decline in the number of Swedish conscripts during the 1970s and 1980s and especially in the 1990s following the fall of the Berlin Wall. In the year 1990, the army could mobilize around 500,000 men, a sharp reduction compared to 1964 despite significant population growth (The Swedish Armed Forces n.d., "Värnplikten genom åren").

2.2 The reorganizations in 2000 and 2004

Through two government defence bills (swe: "Försvarsbeslut"), the first in 2000 and the second in 2004, SAF shifted focus from a military defence capable of countering possible invasions, invasion defence (swe: "Invasionsförsvar"), to a military defence capable to aid in international wars and conflicts, tactical defence (swe: "Insatsförsvar"). In other words, the SAF should not only be dimensioned to defend territorial borders but also contribute to international peace while maintaining a high defence competence and capability. As a consequence, organisational units were merged with other units, some were moved to different locations and many units were shut down entirely (Proposition 2004/05:5). The reorganizations entailed a continuing decline in the number of conscripts until mandatory conscription was abolished completely in 2009, only to be reintroduced again in 2017 (The Swedish Armed Forces n.d., "Dödspatrullen").

The Reorganisation Group (swe: "Arbetsgruppen Grundorganisation"), or as they were infamously nicknamed "The Death Patrol " (swe: "Dödspatrullen"), was a unit of officers within the SAF responsible for deciding which military bases to close during the extensive reorganization of 2004. The group earned their dramatic epithet due to their resolute power, and local papers often wrote articles informing residents about their intended arrival. The Death Patrol reported to the Supreme Commander of the SAF (swe: "Överbefälshavaren") who assigned the group with three budgetary options: (i) To remain the 40 bn SEK military expenditure, (ii) decrease expenditures by 3 bn SEK annually or lastly (iii) decrease expenditures with 6 bn SEK. The first two options would entail layoffs of 2500 military officers. The third option would imply 3600 military officers losing their job - a decrease of Swedish military personnel with 25 %. The group consequently travelled around Sweden visiting almost all organisational units in the Swedish Armed Forces and evaluated what role they could play in the future of SAF's organization (The Swedish Armed Forces n.d., "Dödspatrullen").

The Death Patrol was instructed to consider the following parameters during the reorganization: defence aspects, economical aspects, local political aspects, work environment aspects and

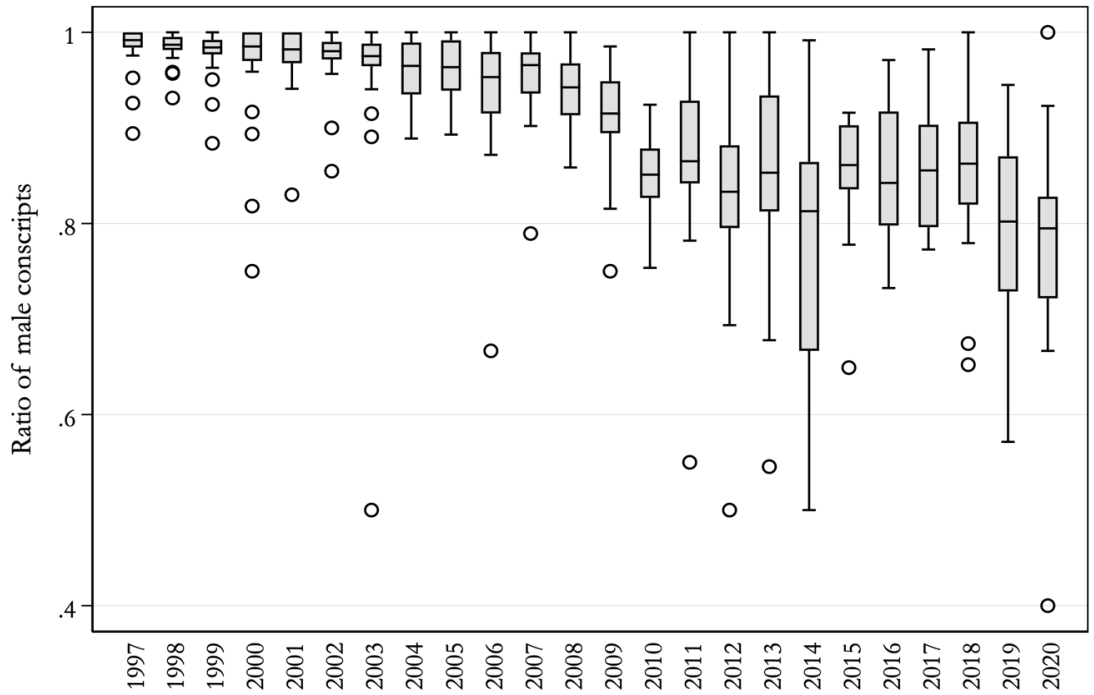
environmental aspects. Local political aspects included local labour markets and how they might be affected by moving or closing the military operations in that area. Special consideration was given to areas where a large portion of the residents was employed by the military base. However, in practice, this consideration was subordinate to the overall main objectives of cost-efficiency and military strategy, such as identifying productivity gains and a long-term organisational structure fit for the new strategic focus (Proposition 2004/05:5).

When accounting for cost-efficiency and military strategy, the geographic locations of the bases were of crucial importance in the decision-making process. On one hand, logistics had to be considered, leading to bases being merged and relocated to become as resources efficient as possible. On the other hand, SAF had to prepare their soldiers for different combat environments, which requires bases along the east and west coasts as well as in different climate typologies. Consequently, the SAF had to balance logistic and economic efficiency with being decentralised geographically for strategic reasons. An example of this balance is the closure of organizational unit "K4" in the small town of Arvidsjaur. Even though K4 had a lot of conscripts at the time, after a period of decline, it was closed to accommodate for the "K3" unit in Karlsborg which was deemed as strategically more important due to its more advantageous geographic location (Proposition 2004/05:5).

2.3 Conscription in Sweden

Conscription service is the mandatory enlistment into military service with legal reprimands for incompliance dating back to 1901 in Sweden. For the majority of the 20th century, nearly all men were summoned for military service. For instance, in 1979, 95 % of all Swedish men were mustered for military service. The remaining 5 % consisted of men physically or mentally unfit for service or of men not considered as Swedish citizens. Of the 95% tested, a majority were called into service (Hjalmarsson and Lindquist 2019). Conscription has historically also been very male-dominated, and Sweden is no exception (Persson and Sundevall 2019). The first woman allowed to conscribe was in 1980 (The Swedish Armed Forces n.d., "Värnplikten genom åren"), and since 1997 the share of male conscripts has centred around 90 per cent annually as seen in Figure 1.

Figure 1: Proportion of male conscripts
In each municipality, yearly 1997-2020



Notes: The spread increases as the total amount of conscript decreases, i.e., the decreased gap between male and female conscripts is rather driven by a decrease in the number of male conscripts as the number of female conscripts has remained largely flat, see Figure 3 (The Swedish Defence Conscription and Assessment Agency 2021). Likewise, the outliers with a rather equal amount of male and female conscripts arise at very low levels of conscription among bases that are in the process of being shut down.

The Swedish conscription selection process consists of three subsequent stages; (i) screening process, (ii) muster and (iii) decision to serve or not. The screening process, overseen by The Swedish Defence Conscription and Assessment Agency (SDCAA), assesses physical health, mental health and, in later years also, the degree of voluntariness. SDCAA also performs a limited criminal background check on all conscripts. This control, based on the Swedish Criminal Records (swe: "belastningsregistret"), evaluates records relating to assault, rape, property crimes and firearm violations. Some conscripts might be subject to further investigation if their conscription will require access to classified material (Björklund 2003). Those qualifying for mustering are more rigorously tested for health factors such as vision, hearing, stamina, strength, cognitive skills through verbal and logical tests and finally for psychological factors. Lastly, an officiator interviews and evaluates the enlistee's test results and decides if it is eligible for service and in which unit (Hjalmarsson and Lindquist 2019).

The screening process has changed in increments during the 1990s and 2000s. In 1995 a new law was passed, The Law of Conscription (swe: "Lagen om totalförsvarsplikt"), which made the screening process more rigorous, resulting in fewer conscripts being called for mustering. In 2007, the SAF also introduced a pre-test via the internet, resulting in even fewer qualifying for the second stage in the testing process. Service is usually between 6-15 months (Hjalmarsson and Lindquist 2019). Generally, conscripts are discharged during weekends and most weekday evenings (The Swedish Armed Forces n.d., "Förmåner och villkor under grundutbildning med värnplikt"). The Law of Conscription applied to all men between 19 and 47 years old, meaning most men were enlisted directly after graduating from secondary school. When mandatory conscription was abolished in Sweden, the law was changed to include women as well (Proposition 2009/10:160).

3 Related Literature

3.1 Local determinants of crime by Paloyo, Vance and Vorell

Paloyo, Vance and Vorell (2014) studied the effect German military bases had on local crime. Like in Sweden, the German military forces were extensively downsized between 2003-2007, leading to nationwide closures of military bases. Since German military personnel primarily consists of young men, a demographic overrepresented in crime, Paloyo et al. hypothesized a reduction in local crime where military bases were decommissioned.

Their empirical methodology is based on a panel dataset consisting of 298 municipalities where 105 municipalities contained military bases that closed during the studied period. They run regressions on the number of reported crimes, in logarithm, with the number of military personnel in thousands as the independent variable. Their model is tested on the following crime categories; total crime, breaking and entering, automobile-related crime, violent crime, and drug-related crime. To control for unobserved municipality-specific and time-specific characteristics, they also add a time-invariant unit fixed effect as well as a vector of unit-invariant year fixed effects. They proceed to apply a fixed-effect transformation to the data to eliminate any residual biases.

To account for the fact that crime may not be committed in the same municipality as the military serviceman resides in, Paloyo et al. include crime committed in a 12 and 20 km radius to the military base. This is carried out by mapping out the radiuses using GIS (geographical information systems) software and creating a weighted sum of crimes based on the radiuses' coverage of each municipality. Furthermore, Paloyo et al. include the following control variables that are seen as traditional determinants of crime; GDP, household relative income, the share of foreigners, the share of men aged 15-25, and the size of the municipality. All in all, they do not find a significant effect for any types of crime due to changes in military presence. Hence, they conclude that local crime should not be taken into consideration in military policymaking.

3.2. Military conscription on post-service crime by Hjalmarsson and Lindquist

While Paloyo et al. use a fixed effects model on a municipal level, Hjalmarsson and Lindquist (2019) use an instrumental variable framework on an individual level. Hjalmarsson and Lindquist study whether conscripted individuals have a higher propensity to commit crime, both during and after service.

The authors study Swedish men born between 1968 and 1983, who consequently were mandated to undergo enlistment test between 1990 and 2001. Since the selection process is based on inherent physical and psychological characteristics of the conscript, simply comparing those who completed conscription with a control group who did not yield an inequitable result. Hence, an IV-variable is constructed based on which officiator a testee is assigned. Hjalmarsson and Lindquist define "high annual service rate" as officiators with a higher-than-average acceptance rate and "low annual service rate" as officiators with lower-than-average acceptance rate. The officiator is exogenously assigned to the testee and is, as such, a suitable IV-variable. Using this method, the authors test if conscription affects post-service crime, specifically convictions between the age of 23-30. The effect is also compared between different socioeconomic groups across two dimensions: if the conscript has a criminal background prior to service and what socio-economic background the conscript belongs to (using the fathers' incomes as a proxy). The authors study the effect on crime

during conscription as well, using both the instrumental variable but also a DiD-framework comparing conscripts to non-conscripts.

Hjalmarsson and Lindquist show that conscription is positively correlated with post-service crime and increases the likelihood to be convicted of a crime post-service by 8,9 percentage points. The effect is primarily driven by individuals from low socioeconomic backgrounds with previous convictions. Moreover, there is evidence of a peer-effect, i.e., ex-convicts socialise during conscription which further reinforces pre-service criminal behaviour.

However, on the contrary, during conscription the authors find a decrease in criminal convictions. They attribute this effect to the conscripts being incarcerated most of the time. The negative causal effect is observed for alcohol and drug-related crime and is true for individuals with and without previous convictions. In other words, they conclude that "Taken together, our…estimates imply that there are significant incapacitation effects of military service. Unfortunately, our analysis also suggests that these effects are not large enough to break a cycle of crime that has already begun prior to service".

3.3 Military service and crime

The relationship between military service and crime is a well-researched subject. However, the literature has mainly focused on how military service affects the *future* likelihood of committing a crime, with studies on Argentina's draft-lottery finding that conscription *increases* the likelihood of developing a criminal record for cohorts both in peace- and wartime (Galiani et al. 2011). Looking at Scandinavia, a longitudinal study of draft-lottery in Denmark found that military service has a *reducible* effect on property crime among youth offenders but not an effect on violent crime (Albæk et al. 2017). And, as mentioned above, Hjalmarson and Lindquist (2019) showed that mandatory military service in Sweden has a significant *positive* causal effect on crime across multiple crime categories, an effect primarily driven by young men from low socioeconomic backgrounds.

Furthermore, a lot of inconclusive research has been conducted on American military bases (Baktir et al. 2020). To explain the ambiguity, Baktir et al. present a theoretical dichotomy in the research: one perspective advocating for lower propensity to commit crime among individuals with military experience, and the other advocating for an increased propensity. The authors write that, according to the first perspective, "...military experience enables individuals to desist from their prior antisocial lifestyle through offering a structured routine, a sense of personal agency, as well as an increase in social capital through education and job training. When the military serves as a turning point in these individuals' lives, they have a much lower tendency for offending (Sampson and Laub 1993; Laub and Sampson 2003)". Whereas, when defining the opposing perspective, they write that "... the military is a crime-aggravating environment. The military teaches its members to behave more aggressively and may put the members in an environment that increases their stress (Bohannon et al. 1995; Hakeem 1946)."

To explain these two opposing effects, Baktir et al. conclude that the direction of the relationship depends on the type of criminal activity; in general, military experience decreases the risk of non-violent crime while it increases the risk of violent crime. The negative relationship supports the first perspective as the military holds its members to strict behavioural standards where stealing or similar non-violent activities are strictly prohibited. The positive relationship supports the second perspective as the military can be an aggressive environment which may increase the propensity to commit violent crime. This increased propensity to commit violent crime among military personnel is also highlighted by Grossman (1995) who argues that exposure to weapons and

general desensitization to violence may lead to an increase in violent crimes, as cited by Hjalmarsson and Lindquist (2019). Finally, when grouping by military status, Baktir et al. also find an incarceration effect where, more often than not, the active-duty military had a lower risk of being arrested or engaging in non-violent crime than non-active.

However, the research into contemporaneously committed crime by active-duty conscripts is less extensive, even though there have been reports on crime committed by conscripts. The Swedish conscription newspaper Värnpliktsnytt (2003) reported that out of the 14 977 people conscripted in January 2003, 9 percent had previously to their conscription been convicted for a felony. Among this was 173 cases of assault and aggravated assault and 280 property crimes. Citing data from The Swedish National Council for Crime Prevention (Swe: "Brottsförebyggande rådet" or "BRÅ"), conscripts have more convictions for assault and shoplifting than the "average population". However, it is important to note that the "average population" in this article includes the entire population of both men and women whereas conscripts are mainly men (see Figure 1); thus, these numbers show an unfair comparison. Furthermore, random drug tests by the Swedish Armed Forces have shown an increase in the use of narcotics among conscripts in the early 2000s (Värnpliktsnytt 2010). Värnpliktsnytt cites research conducted by The Council for Conscripts (swe: "Värnpliktsrådet") which shows that the number of conscripts believing that their comrades use narcotics during their leave of absence has increased from 23 percent, in 2006, to 35% in 2009. Even though this doesn't address the use of narcotics during conscription, it nevertheless highlights that drug abuse is a problem among conscripts as well.

3.4 Local effects by military bases

Given that military bases act as employers in the region they are situated, it's fair to assume that military base closures have an impact on the regions surrounding them.

In the US, research has been conducted on the five rounds of base closures from 1988 to 2005 performed by the Commission on Base Realignment and Closure (BRAC) (Poppert and Herzog 2003; Hultquist and Petras 2012). Poppert and Herzog's results, analysing the closures from 1988 to 1995, question the gloomy conception of long-term employment reduction following base closures. In opposite, the authors find that "the total (direct, indirect, and induced) long-run impacts of BRAC personnel reductions are (overall) positive." Furthermore, they argue that combining this labour effect with that of reutilized infrastructure and direct federal assistance yields a sizable and favourable effect on the labour market and discredit any fear of catastrophic job losses. However, following up on the work by Poppert and Herzog, Hultquist and Petras (2012) find that reductions in military base employment, specifically in enlisted non-civilian personnel, have a statistically significant negative effect on local non-base employment, both within the same county and in neighbouring counties.

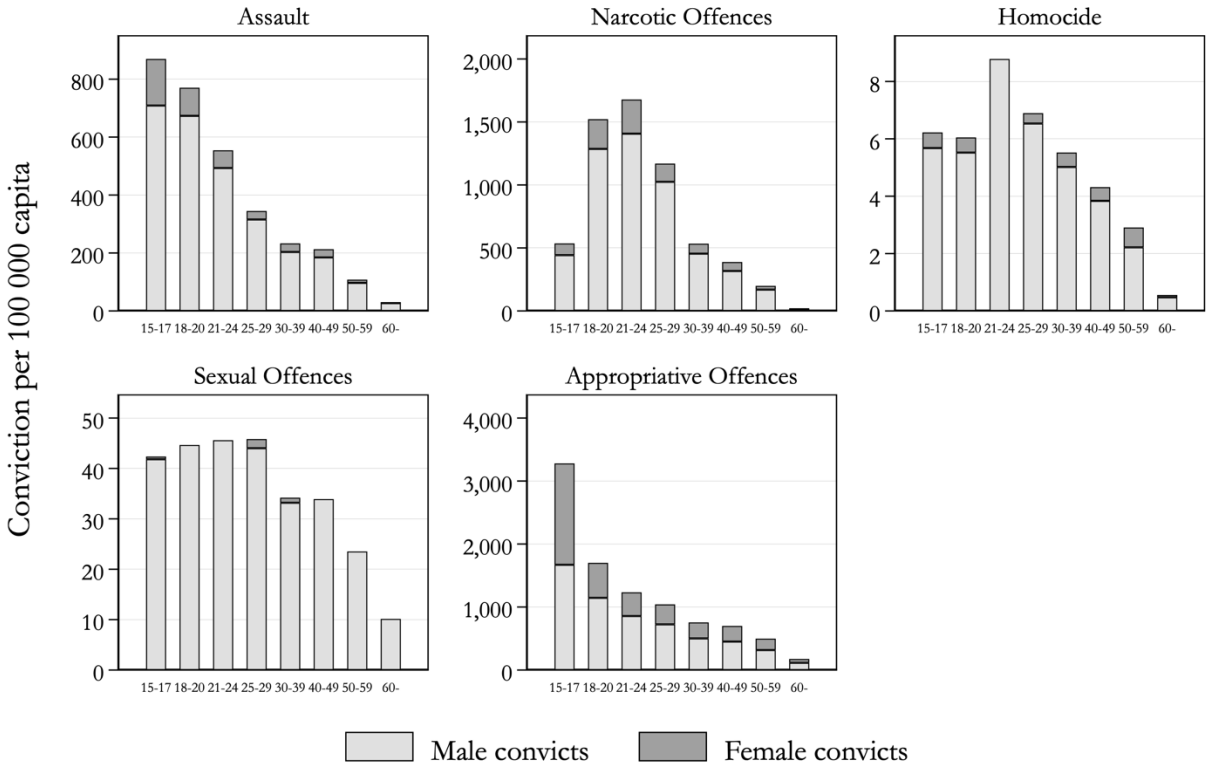
In a Swedish context, the closure of military bases has not had any significant impact on average income or net migration in affected municipalities (Andersson et al. 2007). Andersson et al. use a panel data set of 31 municipalities covering base closures occurring between 1983 and 1998 and attributes the lack of significant impact to the composition of the labour force: "Those previously employed at the military bases have now found new employment in the region, either in the private or the local public sector", the authors states.

From a criminal point of view, in contrast to the insignificant results shown by Paloyo et. al, Anderson and Rees (2015) studied Fort Carson in El Paso County, Colorado US by comparing units that were and weren't deployed in Iraq between 2001 and 2009. They found that an increase

of one never-deployed combat brigade at Fort Carson was associated with an 8.4 percent increase in aggravated assault, an 8.6 percent increase in rapes and a 13.9 percent increase in robberies. These brigades were also associated with a 5.4 percent increase in arrests of 18-29-year-old males for violent crimes. In contrast, the estimates for previously deployed units were generally small and statistically insignificant, with some evidence even showing a negative relationship. However, a Swedish study did not find an increased crime propensity for Swedish soldiers returning from deployment in Afghanistan (Pethrus et al. 2019)

3.5 Crime and young men

Figure 2: All convictions by principal offence
By gender and age group, year 2009



Note: The age group displays the perpetrators' age at the time of conviction (The Swedish National Council for Crime Prevention n.d., "Personer lagförda för brott").

The figures above show a negative correlation for all crime types, among both men and women, between convictions and age, some after a peak in the mid-20s. Furthermore, all crime types show a consistently higher number of male convicts than female convicts. Thus, we can observe that the demographic most prone to commit appropriative, narcotic and violent crime are primarily young men.

The reasons for men being overrepresented in criminal offences are commonly attributed to differences between the sexes in crime profitability, risk aversion, cognitive and non-cognitive abilities, physical strength, aggressiveness, camaraderie, and gender roles (Bäckman et al. 2018). Women, on average, profit 13 percent less than men when committing property crimes, but face

a 9 percent lower probability of arrest (Gavrilovas and Campaniello 2015). Hence, in their study, Gavrilovas and Campaniello attribute 8 percent of the gender gap in criminal propensity to differences in profitability and 56 percent to differences in risk aversion. Furthermore, Kimmel (1997) highlights another inherent gender factor which is the connection between masculinity and violence to which he argues that "...violence is often the single most evident marker of manhood. Rather it is the willingness to fight, the desire to fight". Among Swedish adolescents, the attitude towards violence also differs between young men and women where in general men believe that violence is justified after provocation whereas women believe that violence is never justified (Karlsson and Petterson 2013).

In a social context, studies on peer-effects using Swedish crime data in the early 2000s found a significant peer effect in criminal offences (Lindquist and Zenou 2014). Lindquist and Zenou found that one co-offender increased an offenders' propensity to commit crime by 20% and three co-offenders increased it by 100%. According to the authors, this means that underlying criminogenic factors are exacerbated by this social multiplier effect. Furthermore, Lindquist and Zenou also found that, when forming social links, people tend to sort along income, education, age and country of residence; with age, country of residence and country of origin being the strongest predictor of link formation. This could explain why previously convicted men heavily interacted with other previously convicted men during conscription as reported by Hjalmarsson and Lindquist (2019).

4 Research Question

The literature review above shows that there are significant relationships between future crime convictions and military service, albeit differing in direction. However, the connection between violent crime and conscription is coherently positive in our review, which is consistent with the review done by Baktir et al. as well.

Even though less research has been conducted on crime committed by active-duty military, there seems to be an incapacitation effect preventing servicemen from committing crime during their service. On the other hand, the research on military bases' effect on their local communities gives an inconclusive picture. American studies find a positive effect, in terms of direction, on both employment and convictions (Hultquist and Petras 2012; Anderson and Rees 2015), whereas Swedish studies find no effect at all (Pethrus et al. 2019; Andersson et al. 2007). Paloyo et al.'s (2014) study on the relationship between active-duty military and regional crime, finds no effect either.

However, zooming out of the military context and instead focusing on the demographic composition of the conscripts, it's clear that they mainly consist of people that are statistically most prone to commit a violent felony: young men. As men are more inherently susceptible to violence, putting them in a violent environment, such as a military base during conscription, might cause the violence to manifest itself more easily. Especially given the social multiplicator effect of grouping young men, there's a risk that these underlying factors can be augmented and cause the conscripts to commit crimes they otherwise wouldn't commit in another environment. Furthermore, conscription can also be a place where previous convicts meet each other, as reported by Hjalmarsson and Lindquist (2019) and Värnpliktsnytt (2003).

Hence, by adopting the second part of Baktir et al.'s dichotomy, and as such assuming a positive relationship between military service and crime, as well as focusing on a demographic most prone to commit crime, we ask:

- Is there a positive effect from the number of male conscripts on the number of crimes in the local area?

Furthermore, since the literature has shown differences in what kind of crimes are committed, we also want to analyse that. So, our second question is:

- Does the observed effect differ among different crime types?

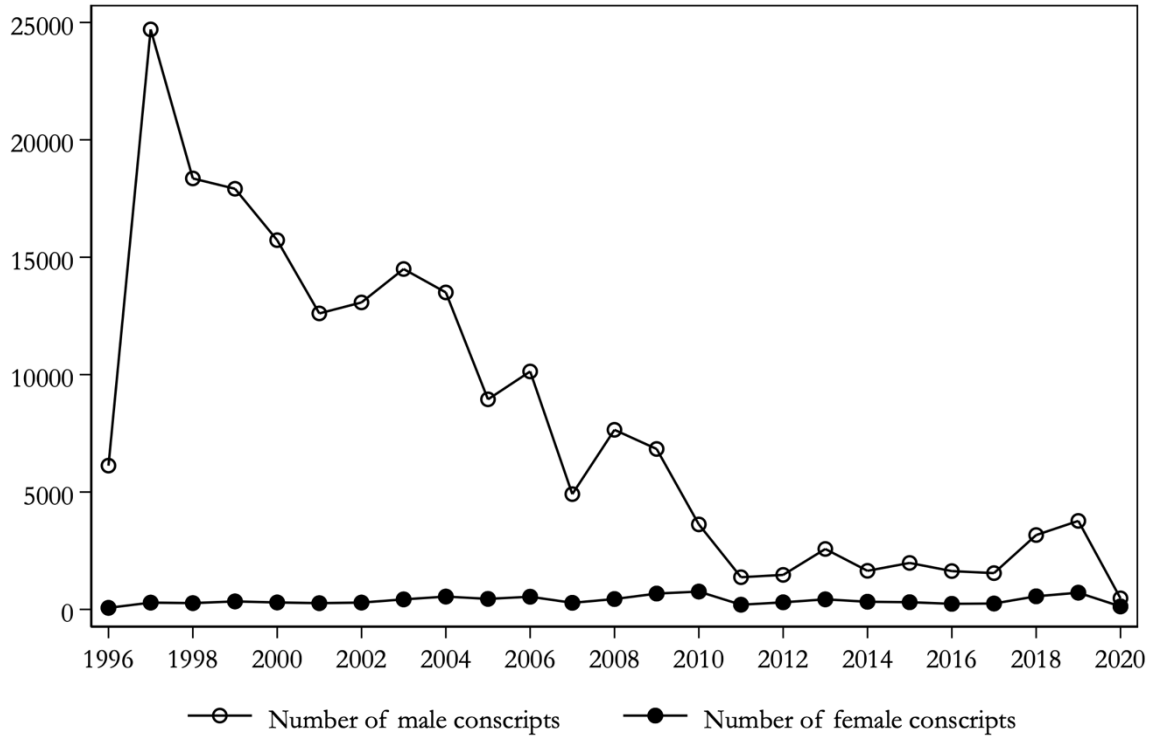
Finally, given that this is, in a sense, a demographic analysis, we assume that the effect should differ between different regions. Specifically depending on the size of the region, where some regions will have a proportionally greater change in their demographics following a change in the number of conscripts than others. So, our last question is:

- Will the observed effects differ depending on the regions' concentration of conscripts?

5 Data

5.1 Data on conscription

Figure 3: Conscripts and other voluntary enlistees
In total, yearly 1996-2020



Note: The graph illustrates the number of male and female conscripts registered at Swedish military bases between 1996 and 2020 (The Swedish Defence Conscription and Assessment Agency 2021).

The number of conscripts on a municipal level has been provided by the SDCAA. The dataset forms a panel of 46 conscript-hosting municipalities (out of 290 in total) between 1996-2020. In 1996 a new system for counting conscripts was introduced, causing a measurement error for that year. The data for the total number of conscripts can be seen in Figure 3.

As outlined under section 2.2, the reorganization in 2000 and 2004 led to multiple base closures and realignments which consequently caused variations in the number of conscripts. Hence, this study will focus on those variations by looking at the four years prior to and after the reorganizations except for the year 1996, i.e., 1997 to 2008. This time period is still relevant for the current setting but does not include extended periods of low numbers of conscripts.

In Figure 6, it's apparent that the development of conscripts is particularly driven by a few municipalities. For example, 13 municipalities did not have more than 100 conscripts at the beginning of the period and 15 municipalities completely lack conscripts after 2001. Furthermore, the dataset confirms that conscription is male dominated, as shown previously in Figure 1 and discussed in section 2.3.

With the diminishing demand for conscripts during the studied period, see section 2.1 and 2.3, the screening process became more rigorous and intensified with an internet pre-test for the two final years in our study. This further emphasized an enlistee's willingness to serve. Consequently, military conscription was arguably more or less voluntary during our studied period. Hence, conscripts should not be mistaken as a random selection of the population of Swedish young men. Quite the contrary, the conscription process selects a homogenous group of men that share a common pool of mental and physical characteristics, have a limited criminal background and a willingness to serve.

5.2 Data on crime

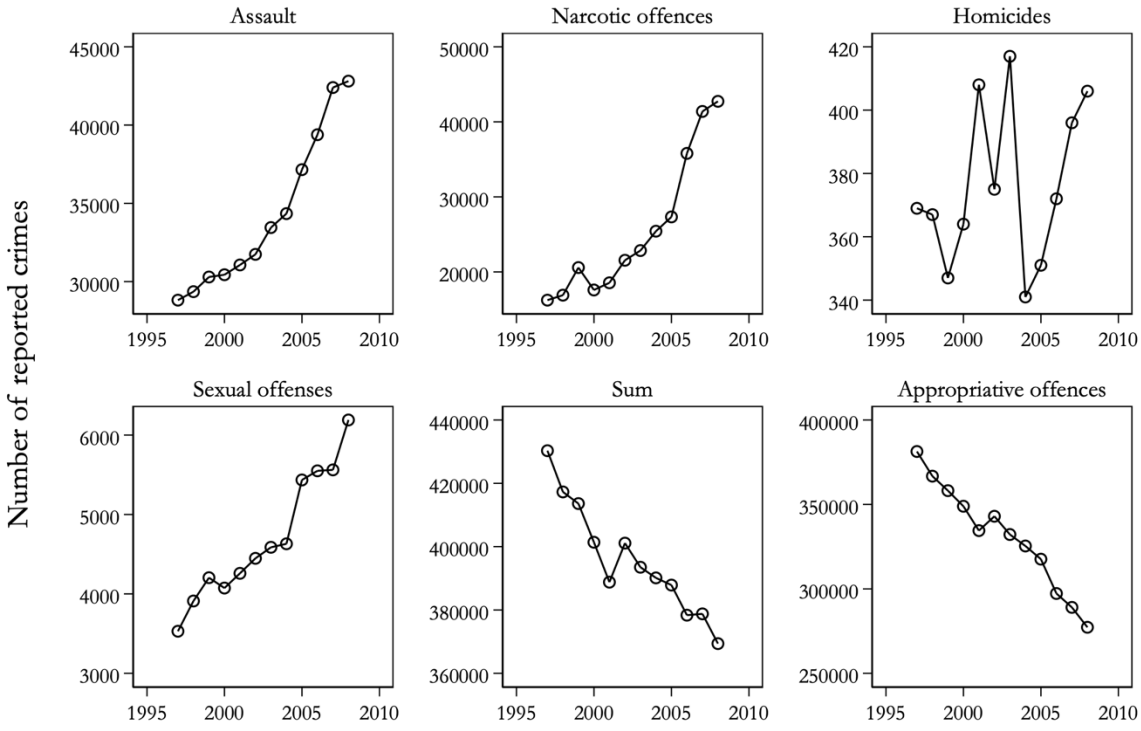
For crime statistics, given the overrepresentation of young men in violent and appropriative criminal offences, highlighted in Figure 2, we have focused on crime regulated in
The Swedish Criminal Code (swe: "Brottsbalken"):

- Homicide: Attempted manslaughter and murder (3rd chapter, 1, 2 §§)
- Assault: Battery and assault (3rd chapter, 5, 6 §§)
- Sexual Offences (6th chapter)
- Appropriative offences: Theft and robbery (8th chapter)
- Narcotic offences: Drug-related crimes ("Narkotikastrafflagen")

The data is exported from The Swedish National Council for Crime Prevention (SNCCP). The SNCCP is the public authority responsible for the official crime statistics, developing knowledge within the criminal justice system and promoting crime prevention work. Our data represents the number of reported crimes for each above-mentioned category on a municipal level between 1997-2008. Figure 4 illustrates the crime development for the studied period for all municipalities included in the study.

Figure 4: Reported offences

In total for the studied municipalities, yearly 1997-2008



Note: The data is provided by the The Swedish National Council for Crime Prevention ("Årsvis - kommun och storstädernas stadsdelar 1996-")

As illustrated, in our municipalities, reported assaults, sexual offences and drug-related crimes have increased. Appropriative offences have decreased, partly driven by a sharp decline in vehicle thefts due to technological advancements making them harder to steal (Nordin and Almén 2011). Reported homicides, on the other hand, vary to a large degree throughout the studied period. The sum of crime exhibits a downward trend, mainly driven by the large proportion of appropriative offences.

A problematic aspect with the crime dataset is the discreteness of the units, causing drastic changes for low levels of crime. For instance, in Boden, there were only 2 reported homicides and 9 reported sexual offences in the year 2000. Furthermore, a few perpetrators can also cause a lot of variation in the data since a single offender may be reported for repeated felonies, such as a repeatedly violent and sexually abusive husband.

Another problematic aspect with reported crimes is hidden statistics which obstructs an apples-to-apples comparison for committed offences across different time periods. Firstly, there is a difference between where and when a crime is *reported* and where and when the crime was *committed*. For example, there is often a long time between sexual perpetration and the ensuing criminal report (Holmberg and Lewenhag 2019), which is attributed to the victim's trauma, sense of fear and shame (Weiss 2010). The perpetration-report-lag creates a time inconsistency in our model, especially when comparing different crime types.

More problematically, a lot of crimes are never reported at all. For instance, sexual offences are not always reported, due to the aforementioned stigma, nor is every narcotic offender caught in action. The SNCCP offers an alternative statistic in their annual survey on victimisation called The

Swedish Crime Survey (SCS, swe: ”Nationella trygghetsundersökningen”). The SCS highlights the disparity between reported and committed crimes by surveying the general Swedish population. Between 2006 and 2011, the survey found a decrease in reported victimisation, while the number of reported crimes had increased (Hagstedt, 2012). In extension, the disparity between reported crimes and committed crimes, i.e., the *report susceptibility*, varies due to: (i) nationwide changes across time and (ii) and municipal differences.

The first implication relates to unit-invariant and time-variant changes that are identical for all municipalities in the panel but varies over time. One such factor is the changes in legal definitions which can lead to more or fewer people being reported for a certain crime. For example, the definition of assault is broader today than 20 years ago. Moreover, cultural factors affecting report susceptibility can change over time. For sexual offences, the report susceptibility grew during the early 2000s due to cultural and political reasons (Hagstedt 2012). Another time-variant factor is the change in police methodology and budgeting. The Swedish Police is centrally organized through governmental directives and regulations which states the organization's overarching goals and focus (The Swedish Police Authority n.d, “Styrning av polisen”). Changes in the allocation of police resources can have an impact on the reported crime statistics. For instance, in the 2000s the Swedish police directed more attention toward crime types detected by surveillance and interventions, leading to substantially more reports on drug and traffic-related crimes than in the 1990s, even though committed crimes likely remained at the same level (Hagstedt 2012).

The second implication relates to time-invariant but unit-variant differences in crime reporting, such as municipalities’ geographic locations and, less evidently, cultural and political differences. The nationwide changes in cultural and political attitude, causing an increased report susceptibility for sexual crimes can, for example, manifest itself differently in a municipality based on its underlying cultural factors. Furthermore, some areas have a local culture of silence (swe: “tystnadskultur”) that affects report susceptibility differently in different areas. For instance, areas with a high presence of gang criminality are less prone to report crimes (Skinnari 2019). This poses an endogeneity issue as it means that previous crime levels, and crime *report* levels, affect future crime report levels, i.e., there is a diminishing marginal report susceptibility over crime. However, as Skinnari points out, this effect emerges in areas with a high presence of gang criminality and, thus, have a limited impact on our model.

Finally, some factors are both unit-variant and time-variant such as unemployment, economic development, and demographic factors.

5.3 Controlling variables

We integrate multiple economic and demographic factors that have been linked to regional crime in previous literature. The variables included are unemployment rate, average annual income, the share of foreign-born, population size and share of young men (15 to 24 years old). The development of the control variables is illustrated in Figure 5.

Unemployment data is gathered from the Swedish Public Employment Service (swe: “Arbetsförmedlingen”) and is measured as the share of people registered at the authority in each municipality. Academic research on the relationship between unemployment and crime is extensive. A Swedish study by Nordin and Almén (2011) finds a strong link between regional long-term unemployment and the number of local violent crimes. This result complements the rich literature on property crime and unemployment in which Ehrlich’s (1973) conclusion, that fewer

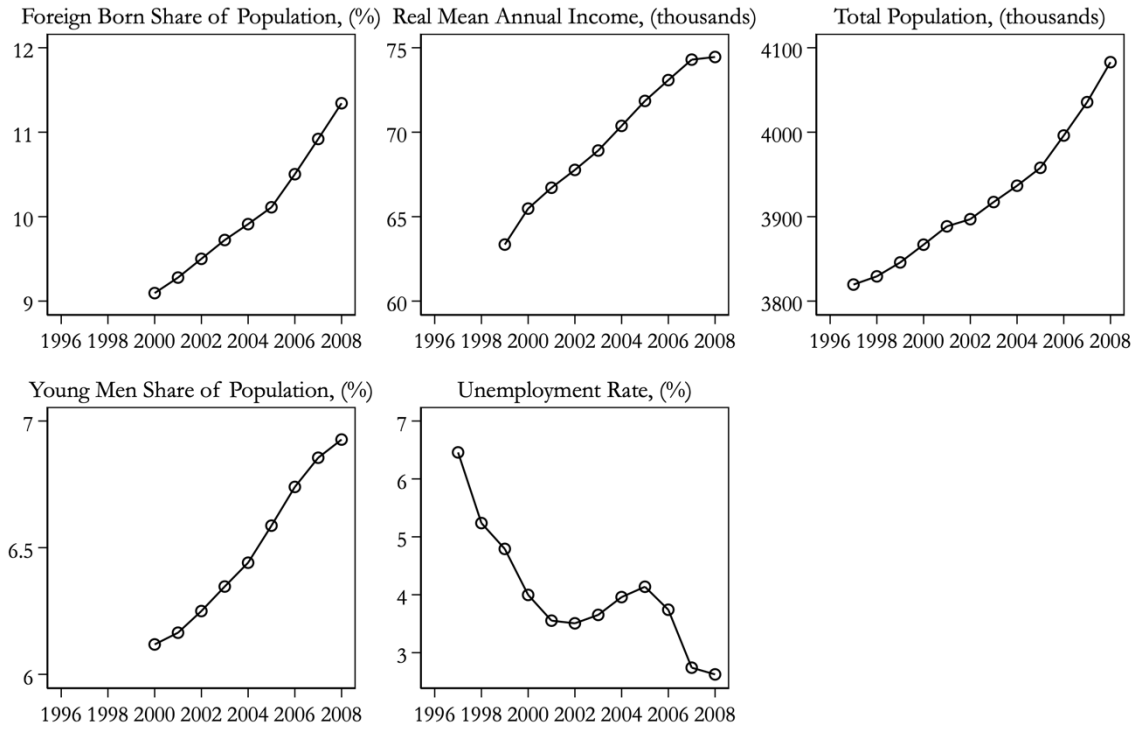
legal labour market opportunities lead to more engagement in the illegal labour market, remains the dominant view today (Nordin and Ahlmén 2011).

Furthermore, the annual average income in each municipality is adjusted for using the Consumer Price Index (CPI) with 1980 as base year. The data is provided by Statistics Sweden (swe: "Statistiska centralbyrån"). Income levels have been shown to negatively affect murder rates (Bharadwaj, 2014).

Data on the share of immigrants is also provided by Statistics Sweden and include individuals born in a foreign country. While a meta-study on crime and immigration by Ousey and Kubrin (2018) find "that overall the immigration-crime association is negative—but very weak.", a Swedish study by Adamsson (2020) finds that individuals with immigrant backgrounds are more likely than average suspects in criminal cases. Likewise, a study by the SNCCP (Backhans and Sundlöf 2019), finds that immigrants are more likely to be suspects of a crime. Lastly, the municipal population is provided by Statistics Sweden.

Figure 5: Control variables

In total for the studied municipalities, yearly 1997-2008



Note: Mean real income is missing values for 1997 while the population of foreign-born and young men share of population miss values for 1997, -98 and -99 (Statistics Sweden (a)-(e); Swedish Public Employment Service)

6 Method

6.1 The model

To analyse the relationship between conscripts and crime, we use an altered version of Paloyo et al.'s model by replacing military personnel with conscripts, and estimate the following regression model:

$$y_{it} = \alpha_i + \tau_t + \delta \text{Conscripts}_{it} + \Phi \mathbf{X}_{it} + \epsilon_{it}$$

Where y_{it} denotes the number of reported crimes, for all subcategories defined in section 5.2 and their sum, in year *t* for municipality *i*, Conscripts_{it} is the number of male conscripts and $\mathbf{X}_{it}$ is a vector of control variables mentioned in section 5.3. τ_t is a time period fixed effect and control for the nationwide time-variant implications on report susceptibility, as discussed in section 5.2. Using an F-test on τ_t over all time periods shows that the time regressor is significant for all crime parameters except for sexual offences, as presented in Table 3. To account for the second implication on report susceptibility, the municipal differences, we also add a municipality fixed effect α_i. ϵ_{it} is the idiosyncratic error term in year *t* for municipality *i*.

6.2 Fixed effects

To account for fixed effects, it is key that strict exogeneity holds, especially with regards to the base closures, discussed in section 2.2, which had a profound impact on the level of conscripts. When deciding on the bases to dismantle, The Death Patrol accounted for "local politics", which could have included local levels of criminality. If criminality had an impact on base closures, it would also, in turn, have an impact on conscript levels and create an endogeneity issue in our model. However, as noted previously, even though the group were to take local politics into account, the main criteria were budgetary and strategic as the SAF was transitioning from invasion defence to tactical defence. Hence, their decisions were primarily based on cost efficiencies and geographic positioning and could not be overruled by the local community, meaning that the endogeneity issue seems unlikely.

Thus, assuming strict exogeneity holds, we use a Hausman test to test whether a fixed effect model is indeed preferable to a random effect model. A Hausman test tests whether the idiosyncratic errors are correlated with the regressors against the null hypothesis that they are not, where a rejection of the null hypothesis supports the choice for a fixed effect model instead of a random effect. In Table 4 our test yields p-values that indicate a correlation between u_{it} and the other regressors. As such, the test indicates that a fixed effect model is more effective than a random effect (Torres-Reyna, 2007). Furthermore, a random effect model assumes that the fixed effect a_i is uncorrelated with the other regressors. This is highly unlikely since fixed effects characteristics played a large role in the decisions underpinning the reorganisations in 2000 and 2004, such as the geographical and strategic position of the bases, which in turn affected the number of conscripts. Fixed effects are also likely to affect the economic development in a municipality and cause changes in employment and our other controlled variables. Hence, a random effect model is discarded as an alternative. We will also regress a first difference model and a pooled OLS model to see how they deviate from our fixed effect model.

6.3 The standard errors

Even though heteroskedasticity and autocorrelation do not affect the consistency of our estimators when using a fixed effect model, they still affect the standard errors and in turn our t and F statistics. When running a Wooldridge test for autocorrelation, all regressions, except for sexual offences and homicide, tests positive for autocorrelation, as shown in Table 5. Furthermore, when running the modified Wald test for groupwise heteroskedasticity, all our regressions test positive for heteroskedasticity, see Table 6. To control for this, we use robust standard errors. Still, given that robust standard errors only follow the t-distribution asymptotically, with a tendency towards downward bias, we might get false-positive results as our dataset is not "large enough" in relation to our time period, N=46 and T=12 (Wooldridge 2016). However, comparing our cluster-robust standard errors with non-robust standard errors shows that they are usually larger; thus, following econometric prudence, only cluster-robust errors will be presented in the paper but questioned in uncertain cases.

To evaluate if there is any multicollinearity in our model, we run a correlation matrix on Conscripts and our controlling variables **X**. In the matrix, Table 7, we can see that there is some degree of multicollinearity in our model, especially for unemployment rate and income as well as population and share of foreigners. However, overall, the collinearity is moderate and limited only to the controlling variables, meaning that the standard errors for our variable of interest remain virtually unaffected. Thus, we do not see multicollinearity as a problem in our model.

6.4 Missing data

The data on the number of conscripts does not contain observations for each municipality in each year. Instead, it only includes a municipality for a specific year if there are any conscripts present in that specific year. As such, missing values for each municipality are treated as 0, i.e., if a municipality is not presented in year *t* that's because they had no conscripts in that year.

For our controlling variables listed in section 5.3, some miss observations between 1997 and 1999 since Statistics Sweden did not record those variables on a municipal level in that period. This omits our time dummies from our regression for those years due to multicollinearity. To control for this, we could have used data that exists on regional levels (swe: "Län") as a controlling variable instead, however, this would pool many municipalities together and result in a significant drop in within-variation and explanatory power for those control variables. Instead, to capture the within-variation among the municipalities, the data is trended by using the compound annual growth for the available period and using that to estimate numbers for the missing years.

To address our last research question and analyse the concentration of conscripts, we simply divide 1997's level of conscripts with the municipal population and use this conscript-to-population ratio as a cut-off point. Then we rerun the regression on different cut-off points to see how the coefficients change and if there are different effects in municipalities with a higher concentration of conscripts. This method will remove some noise in the data set, for example, it is fair to assume that 1500 extra conscripts in Stockholm, a municipality with a low conscript-to-population ratio, will have a negligible effect on the city's overall crime levels.

7 Analysis

Table 1: Summary of Fixed Effect Regression Results for All Municipalities

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.0991*	-0.210*	-0.00302	-0.0233	0.242	-0.0940
	(0.0427)	(0.0929)	(0.00169)	(0.0129)	(0.210)	(0.219)
Unemployment rate	17.01	12.12	0.612	2.914	-49.88	-17.23
	(13.04)	(23.17)	(0.511)	(3.302)	(98.16)	(93.54)
Real income	-19.71*	-41.17*	-0.197	-3.404	43.30	-21.18
	(7.827)	(17.43)	(0.138)	(2.198)	(64.56)	(70.40)
Population	0.0343***	0.0791***	0.000217	0.00671***	-0.280***	-0.159***
	(0.00542)	(0.00983)	(0.000301)	(0.000771)	(0.0224)	(0.0271)
Share of foreigners	9.262	-7.632	0.121	-1.283	99.03	99.50
	(14.33)	(25.41)	(0.527)	(5.643)	(154.7)	(153.7)
Share of young men	27.34	6.499	-0.580	3.560	-69.96	-33.15
	(22.29)	(46.83)	(0.546)	(4.739)	(192.6)	(194.2)
N	552	552	552	552	552	552
adj. R^2	0.767	0.743	0.055	0.603	0.771	0.480
Number of groups	46	46	46	46	46	46

Notes: The units for unemployment, share of foreigners and share for young men are percentage points and should not be confused as semi-elastic elements. Real income is denoted in thousands SEK. Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1 shows the effects of conscription on crime when including all municipalities in the dataset. The coefficients for male conscripts, δ, measure the average increase in reported crime when one extra male conscript is added to a municipality. Two crime types have statistically significant *negative* coefficients for conscripts, assaults and narcotic offences. This means that, on average, one extra male conscript will lead to around 0.1 fewer assaults reports and 0.21 fewer reports of drug abuse. Our pooled OLS model confirms the insignificant coefficients but also yields a *positive* effect on assault, as seen in Table 8. However, we barely consider this effect since the OLS model includes biases from the previously discussed time-invariant unobservable variables. The first difference model in Table 9 doesn't yield any statistically significant effects at all, and the directions are mostly consistent with the fixed effect model. All in all, the all-municipality model generates mostly insignificant and some negative effects from conscripts, none of which are in line with our first hypothesis.

Among the controlling variables, the most noticeable coefficients are those for population where the coefficients are significant on the 0.1% level in all cases but homicides. The positive directions are logical given that an increase in people also increases the number of possible offenders and victims. For instance, one extra person in a municipality increases the number of reported assaults by around 0.034 on average. However, a remarkable effect is the negative direction for appropriative offences which, if true, means that more people lead to fewer thefts and robberies.

The same direction is generated by the first difference model as well but not by the pooled OLS model which shows a significant *positive* effect at the 0.1% significance level. However, the pooled OLS model does not account for developments within units so larger municipalities with more crime naturally create a positive trend. The negative effect from the fixed effect and first difference model is probably merely due to correlation, population has been going up and appropriate crimes down, and not causation. Lastly, real income is negatively associated with assault and narcotic offences at a 5% significance level, but, apart from that, we do not reproduce the effects shown by any of the studies presented in section 5.3. Nor do we, more importantly, see a significant positive effect from the share of you men as expected in section 4.

Additionally, we test the model without any controlling variables which result in the same directions for the coefficients, except for the sum of crimes, but none are significant (Table 10). Notably, the adjusted R^2 are significantly lower for all crime types confirming the usefulness of our controlling variables.

Table 2: Summary of Regression Results for "Base Municipalities"
At different cut-off points for the conscript-to-population ratio

Conscript-to-population ratio	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
1% level	-0.0262	-0.0742*	-0.0000399	-0.00500	0.0389	-0.0665
	(0.0168)	(0.0297)	(0.000394)	(0.00542)	(0.107)	(0.115)
2% level	-0.00952	-0.0409	0.000304	-0.000609	0.0304	-0.0203
	(0.0225)	(0.0371)	(0.000336)	(0.00287)	(0.0987)	(0.0839)
3% level	-0.0158	-0.0461	0.000235	-0.00195	0.197*	0.133
	(0.0220)	(0.0377)	(0.000454)	(0.00311)	(0.0655)	(0.0709)

Notes: The percentage levels indicate the conscripts in proportion to the entire municipal population for the year 1997. I.e., the "x% level" describes a model that includes all municipalities where conscripts corresponded to x% or more of the entire population in 1997. The full output for these models can be found in Table 11-13. Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

When focusing on municipalities with a larger proportion of conscripts per capita, two conscript coefficients are statistically significant:

- -0.0742 at a 5% significance level for narcotic offences in municipalities with conscripts amounting to 1% of their population in 1997
- and 0.197 at a 5% significance level for appropriative offences in municipalities with conscripts amounting to 3% of their population in 1997

In other words, the significant effect for assault disappears, the effect on narcotic offences diminishes and a new effect on appropriative offences appears as more municipalities are filtered from the model. Namely, no clear pattern emerges as we reduce that sample and focus on municipalities where conscripts should have a proportionally larger impact on the area. Instead, the coefficients remain virtually insignificant and the few significant results most probably arise by chance due to skewed standard errors when applying robustness at low levels of N. Hence, we cannot conclude any observed differences depending on the regions' concentration of conscripts.

8 Discussion

Altogether, we see weak and varying directions for the conscript coefficient δ. The ambiguous results are in line with the overall inconsistency in the literature on military service and crime as outlined in section 3. A positive coefficient suggests that an increase in conscripts enlisted in a municipality would lead to an increase in reported crimes, as hypothesized in our first research question. However, in our all-municipality model, we see that the number of conscripts has a statistically significant *negative* effect on assault and narcotic offences specifically. This implies that an increased military presence could have a deterrent policing effect on the inhabitants. Although, this effect has not been recorded in our literature review and seems unreasonable given that military personnel are restrained to their compounds. Another explanation could be due to conscripts acting as a proxy for local military activity, leading to more labour market opportunities (Hultquist and Petras 2012), which in turn leads to less criminal activity (Nordin and Almén 2011). Except for a positive effect on appropriative offences, concentrating the sample to a selection of municipalities with high conscript-to-population does not yield any significant results.

More likely, the inconclusive results and negative coefficients probably arise due to limitations in our data and empirical method. Firstly, as discussed in section 5, the variations in conscripts and our controlling variables are small in many municipalities which limits our model's explanatory power. Secondly, since we run 24 different regressions of our model, it would be surprising to not find any significant results of δ simply by chance, especially given our low number of panel groups.

Moreover, some of the coefficients for our controlling variables contradict previous research. In our all-municipality model, unemployment rate and the share of immigrants are insignificant explanatory variables for crime, in contrast to previous findings (Adamson 2020; Backhans and Sundlöf 2019; Nordin and Almén 2017). The average real income is also insignificant across many cases, except for assault and narcotic offences, contradicting Bharadwaj (2014). Even the share of young men in the municipality is not significant in any regression. These results should not be interpreted as a contribution to the existing research literature on these subjects, but rather supports the argument that our model is flawed and that our data contains noise, which makes our empirical findings on crime imprecise.

Furthermore, when introducing cut-off points to analyse the "base municipalities", naturally, fewer and fewer municipalities are included in the sample. In the all-municipality model, 46 municipalities are included while in the 3% conscript-to-population cut-off only 7 municipalities fit the criteria. With such a small number of groups, our standard robust errors become unreliable, yielding unreliable confidence intervals and question the few significant results we see for the base municipalities.

As thoroughly discussed in section 5.2, the reported crime data from SNCCP entails a disparity between *reported* and *committed* crime due to report susceptibility. Even though some changes in report susceptibility are accounted for, in the time and unit fixed effects, there may still be changes unaccounted for in the model and included in the idiosyncratic errors (as seen in the adjusted R^2 spanning from 0.055 to 0.767 in the all-municipality model). Hence, the unaccounted changes in report susceptibility may create distortions over time which could explain why our coefficients are insignificant. One such unaccounted effect is local multi-offenders causing drastic variations in reported crime, especially in municipalities with low levels of crime.

Nonetheless, after commenting on some of the limitations of the research, the results are still useful. The conscript coefficients, δ, are in all samples generally small and mostly insignificant as shown by the confidence intervals that span over both positive and negative values. Given this property, we can at least conclude that the effect of conscription on crime is relatively small. If the actual effect is very large, despite the problems discussed above, we should have seen it with the method above. In other words, our results make us dismiss our first and second hypothesis that crime should increase with the number of male conscripts due to a demographic shift. Our third hypothesis, that the effect should be larger for base municipalities, is also dismissed, since, as discussed above, no significant effect was found for those either. All in all, our results are in line with the conclusions drawn by Paloyo et al. (2014).

However, the question remains as to why no clear effect is found, despite conscripts consisting of a demographic overrepresented in criminal activity as outlined in section 3.3. One reason behind these insignificant results could be an incapacitation effect, i.e., conscripts are bound to their respective military base, limiting their ability to commit crime, as outlined in previous research (Hjalmarsson and Lindquist 2019; Baktir et al. 2020). Furthermore, when conscripts are on leave, they might depart from the municipality to travel home or to a bordering municipality. Hence, they may still be committing crime during their leave, although not in the vicinity of their military base. Lastly, the conscription process is not random but conversely, it selects a homogenous group of men that share a common pool of mental and physical characteristics, have a limited criminal background and a willingness to serve. This group may have a low criminal profile despite belonging to a gender and age demographic generally prone to commit crime. This coupled with the strict, disciplined and routine environment, may further reduce the conscripts' propensity to commit crime, in line with Baktir et al.'s (2020) findings.

To summarise, the effect of conscription on regional crime is small. Hence, it is fair to assume that it should not have any military policy implications and be accounted for as an indirect cost in the ongoing expansion of the Swedish conscription program. However, the long-term effects are unclear, as Hjalmarsson and Lindquist (2019) find an incapacitation effect on crime during conscription but an increased propensity for crime in the long run.

9 Further Research Implications

As outlined above, our data on conscripts and crime are the main limitations for making any definitive conclusions to our research questions. Both the SNCCP and Statistics Sweden have non-public individual data, providing an array of alternative empirical methods, such as directly analysing convictions for felonies committed during conscription. These convictions could then be compared with non-conscript convictions, like the DiD-framework constructed by Hjalmarsson and Lindquist (2019).

Moreover, as discussed, the number of observations is low, especially when municipalities are excluded based on their concentration of conscripts. Therefore, choosing a different country with more conscript-hosting municipalities, creating a larger sample size, would have been more effective. Alternatively, one could study Swedish conscription during previous time periods when conscription was more widespread, although, municipal crime data is not publicly available prior to 1996.

Additionally, in our method, we define the "local area" by municipal borders. For instance, this implies that our model does not account for crime committed in Stockholm by conscripts stationed in the adjacent municipality Vaxholm. Thus, a more proper methodology would be to use GIS software and create a weighted sum of local crime like Paloyo et al. (2014).

Finally, adding other criminogenic factors, such as education and GDP, to the regression would create more accurate coefficients. Additionally, some of the current control variables in the regression could have been lagged to create more accurate coefficients. For instance, changes in unemployment probably do not affect crime immediately, as assumed in our model. Lagging the control variables would not affect the efficiency of δ, since they do not correlate with conscription, but mainly increase the precision of the overall regression.

References

Adamson, G. (2020). Migrants and crime in Sweden in the twenty-first century. *Society, 57*(1), 9-21. doi:10.1007/s12115-019-00436-8

Albæk, K., Leth-Petersen, S., le Maire, D., & Tranæs, T. (2017). Does peacetime military service affect crime? *Scandinavian Journal of Economics, 119*(3), 512-540. doi:10.1111/sjoe.12181

Anderson, D. M., & Rees, D. I. (2015). Deployments, combat exposure, and crime. *The Journal of Law & Economics, 58*(1), 235-267. doi:10.1086/682907

Andersson, L., Lundberg, J., & Sjöström, M. (2007). Regional effects of military base closures: The case of Sweden. *Defence and Peace Economics, 18*(1), 87-97. doi:10.1080/10242690600924638

Backhans, M., & Sundlöf, P. (2019). *Nordiska studier om brottslighet bland personer med utländsk och inhemsk bakgrund - en kartläggande litteraturöversikt av publicerad forskning och statistik 2005–2019*

Bäckman, O., Hjalmarsson, R., Lindquist, M. J., & Pettersson, T. (2018). Könsskillnader i brottslighet - hur kan de förklaras? *Ekonomisk Debatt, 46*(4), 67-78.

Baktir, Y., Mustafa Icer, M., & Craig, J. (2020). Military and crime: A systematic review of the literature. *Deviant Behav., 41*(2), 233-251. doi:10.1080/01639625.2018.1556997

Bharadwaj, A. (2014). Is poverty the mother of crime? Empirical evidence of the impact of socioeconomic factors on crime in India.*Atlantic Review of Economics, 1* Retrieved from http://www.econstor.eu/dspace/Nutzungsbedingungen database. Retrieved from http://hdl.handle.net/10419/146581

Björklund, L. (2003). *Försvarets personkontroll - Svar på skriftlig fråga 2003/04:23*. Retrieved Apr 14, 2021, from https://www.riksdagen.se/sv/dokument-lagar/dokument/svar-pa-skriftlig-fraga/forsvarets-personkontroll_GR1223

Doyle, J. J., & Aizer, A. (2013). *Juvenile incarceration, human capital and future crime: Evidence from randomly-assigned judges* National Bureau of Economic Research. doi:10.3386/w19102

Galiani, S., Rossi, M. A., & Schargrodsky, E. (2011). Conscription and crime: Evidence from the Argentine draft lottery. *American Economic Journal: Applied Economics, 3*(2), 119-136. doi:10.1257/app.3.2.119

Gavrilova, E., & Campaniello, N. (2015). *Uncovering the gender participation gap in the crime market*. Bonn; IZA: Institute for the Study of Labor. Retrieved from http://www.econstor.eu/dspace/Nutzungsbedingungen database. Retrieved from http://hdl.handle.net/10419/110713

Grossman, D. A. (1995). *On killing: The psychological cost of learning to kill in war and society* (1. ed. ed.). Boston [u.a.]: Little, Brown and Company.

Hagstedt, J. (2012). *Den svenska brottsutvecklingen*. The Swedish National Council for Crime Prevention. Retrieved from: https://www.bra.se/download/18.22a7170813a0d141d21800052644/1371914741466/01+Den+svenska+brottsutvecklingen.pdf

Hjalmarsson, R., & Lindquist, M. J. (2019). The causal effect of military conscription on crime. *The Economic Journal (London), 129*(622), 2522-2562. doi:10.1093/ej/uez014

Holmberg, S., & Lewenhagen, L. (2019). *Våldtäkt från anmälan till dom - en studie av rättsväsendets arbete med våldtäktsärenden.* Retrieved from: https://www.bra.se/download/18.62c6cfa2166eca5d70e18f68/1615395291012/2019_9_Valdtakt_fran_anmalan_till_dom.pdf

Hultquist, A., & Petras, T. (2012). An examination of the local economic impacts of military base closures. *Economic Development Quarterly, 26*, 151-161. doi:10.1177/0891242412442374

Karlsson, J., & Pettersson, T. (2003). *Fokusgruppsintervjuer med ungdomarom genus och våld : Konstruktioner av gärningspersoner och offer*. Stockholm: Kriminologiska institutionen, Stockholms universitet.

Keller, K., Poutvaara, P., & Wagener, A. (2010). Does a military draft discourage enrollment in higher education? *FinanzArchiv / Public Finance Analysis, 66*(2), 97-120. Retrieved from http://www.jstor.org/stable/40913249

Kimmel, M. S. (1997). Masculinity as homophobia: Fear, shame and silence in the construction of gender identity. (pp. 223-242). Florence, KY, US: Taylor & Frances/Routledge. Retrieved from RIS Format UTF-8

Lag (1994:1809) Om Totalförsvarsplikt, https://www.Riksdagen.Se/Sv/Dokument-Lagar/Dokument/Svensk-Forfattningssamling/Lag-19941809-Om-Totalforsvarsplikt_sfs-1994-1809,

Lindborg, A. (2015, 11-09-). Låt värnplikt bidra till bra integration. *Aftonbladet,* Retrieved from https://www.aftonbladet.se/ledare/a/rLVR68/lat-varnplikt-bidra-till-bra-integration

Lindquist, M. J., & Zenou, Y. (2014). *Key players in co-offending networks*. Bonn; IZA: Institute for the Study of Labor. Retrieved from http://www.econstor.eu/dspace/Nutzungsbedingungen database. Retrieved from http://hdl.handle.net/10419/93360

Lissheim Lennart. (2003, Oct 7,). Säkerhetskontrollen har haft stora brister. *Värnpliktsnytt,*

Nordin, M., & Almén, D. (2017). Long-term unemployment and violent crime. *Empirical Economics, 52*(1), 1-29. doi:10.1007/s00181-016-1068-6

Ousey, G. C., & Kubrin, C. E. (2018). Immigration and crime: Assessing a contentious issue. *Annual Review of Criminology, 1*(1), 63-84. doi:10.1146/annurev-criminol-032317-092026

Paloyo, A., Vance, C., & Vorell, M. (2014). Local determinants of crime: Do military bases matter? *Demographic Research, 31*(21), 625-658. doi:10.4054/DemRes.2014.31.21

Persson, A., & Sundevall, F. (2019). Conscripting women: Gender, soldiering, and military service in Sweden 1965–2018. *Null, 28*(7), 1039-1056. doi:10.1080/09612025.2019.1596542

Regeringens Proposition 2004/05:43 - Vårt Framtida Försvar, (2004). Retrieved from: https://www.regeringen.se/49bb68/contentassets/0377273a4b9a41f7938360a6a00e82d4/prop.-20040543-forsvarsmaktens-grundorganisation?TSPD_101_R0=088d4528d9ab20003cf5ca78bc7147319c9fc648609950254834cca4b8eb90097e262c52a7d03df208d8e48dfa14300047a0b352ebbba9866eab7c330f70ef42dab4a72d806cd9e97c4b11cafe055bb10dca5c3f588c3593f8a749132ac226a7

Peter Walleberg. (2020, 12-15-). ÖB: En rejäl satsning. *Tt,* Retrieved from https://app.retriever-info.com/go-article/05080620201215httpttsemediatext201215forsvarsbeslutuv2cc4822b5/null/archive/search?type=jwt

Pethrus, C. M., Frisell, T., Reutfors, J., Johansson, K., Neovius, K., Söderling, J. K., et al. (2019). Violent crime among Swedish military veterans after deployment to Afghanistan: A population-based matched cohort study. *International Journal of Epidemiology, 48*(5), 1604-1613. doi:10.1093/ije/dyz084 [doi]

Poppert, P. E., & Herzog Jr, H.,W. (2003). Force reduction, base closure, and the indirect effects of military installations on local employment growth. *Journal of Regional Science, 43*(3), 459-482. doi:10.1111/1467-9787.00307

Regeringens Proposition 2009/10:160 - Modern Personalförsörjning För Ett Användbart Försvar Prop. - Vissa Frågor Om Försvarsmaktens Personal, (2010). Retrieved from: https://data.riksdagen.se/fil/0534B316-EECB-42D2-9EEA-06F9C6768130

Skinnari, J., Jonsson, A., & Vesterhav, D. (2019). *Tystnadskulturer en studie om tystnad mot rättsväsendet*

Statistics Sweden. (a). *Folkmängden efter region, civilstånd, ålder och kön. år 1968 - 2020*. Retrieved April 01, 2021, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__BE__BE0101__BE0101A/BefolkningNy/?fbclid=IwAR0vPnPvjHaQNX82EWdWiwI126DRcmZ-CB58NCHWT33Sn8QmTgXgGfmpV_s

Statistics Sweden. (b). *Inrikes och utrikes födda efter region, ålder och kön. år 2000 - 2020*. Retrieved April 25, 2021, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__BE__BE0101__BE0101E/InrUtrFoddaRegAlKon/?fbclid=IwAR3-ccNL6EtUh59vWo4zo-VYztex9Hr8gF2xMj2ZobzbW0vxSrN6GsqAiPs

Statistics Sweden. (c). *Konsumentprisindex (KPI) årsmedeltal totalt, skuggindextal, 1980=100. år 1980 - 2020* . Retrieved April 25, 2021, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__PR__PR0101__PR0101A/KPISkuggAr/?loadedQueryId=81149&timeType=from&timeValue=1980&fbclid=IwAR01qlaU3qq3hBdoZBvDuxHuizlYwhRcCGFyVZD7OE7E6PkyLC3DzWXm31o

Statistics Sweden. (d). *Medelfolkmängd (ålder under året) efter region, civilstånd, ålder och kön. år 2006 - 2020*. Retrieved April 18, 2021, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__BE__BE0101__BE0101D/MedelfolkHandelse/#

Statistics Sweden. (e). *Sammanräknad förvärvsinkomst för boende i sverige hela året efter region, kön, ålder och inkomstklass. år 1999 - 2019* . Retrieved Mar 29, 2021, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__HE__HE0110__HE0110A/SamForvInk1/?fbclid=IwAR3vC4_wXD1QH_paH7YT534wdq7voF-xFghsjnvce9jWPwn-FKP0odepkhs

Swedish Public Employment Service. *Tidigare statistik*. Retrieved April 13, 2021, from https://arbetsformedlingen.se/statistik/sok-statistik/tidigare-statistik?fbclid=IwAR0GFHx2ZR2KrxB0vnxd1wVpw_ar5bWi4pazpITtGDQkTgboJ6UO-6WhtS4

The Swedish Armed Forces. (a). *"Dödspatrullen" - historien bakom försvarsbeslutet 2004*. Retrieved Apr 17, 2021, from https://www.forsvarsmakten.se/sv/information-och-fakta/var-historia/artiklar/dodspatrullen/

The Swedish Armed Forces. (b). *Förmåner och villkor under grundutbildning med värnplikt*. Retrieved Apr 15, 2021, from https://jobb.forsvarsmakten.se/sv/utbildning/grundutbildning/villkor/

The Swedish Armed Forces. (c). *Värnplikten genom åren*. Retrieved May 13, 2021, from https://www.forsvarsmakten.se/sv/information-och-fakta/var-historia/artiklar/varnplikt-under-109-ar/

The Swedish Defence Conscription and Assessment Agency. (2021). *Antal inryckta värnplikt eller frivillig gmu 1996-2020 per kommun kön*. Retrieved April 22, 2021, from Karlstad: The Swedish Defence Conscription and Assessment Agency

The Swedish National Council for Crime Prevention. (a). *Årsvis - kommun och storstädernas stadsdelar 1996-* . Retrieved April 27, 2021, from http://statistik.bra.se/solwebb/action/index

The Swedish National Council for Crime Prevention. (b). *Personer lagförda för brott*. Retrieved April 03, 2021, from https://www.bra.se/statistik/kriminalstatistik/personer-lagforda-for-brott.html

The Swedish Police Authority. (2020). *Styrning av polisen*. Retrieved April 05, 2021, from https://polisen.se/om-polisen/uppdrag-och-mal/styrning-av-polisen/

Torres-Reyna, O. (2007). *Panel data analysis fixed and random effects using stata (v. 4.2)*. Retrieved May 3, 2021, from https://www.princeton.edu/~otorres/Panel101.pdf

Wännström Olle. (2010, Jun 08,). Fler soldater knarkar. *Värnpliktsnytt,* pp. 5.

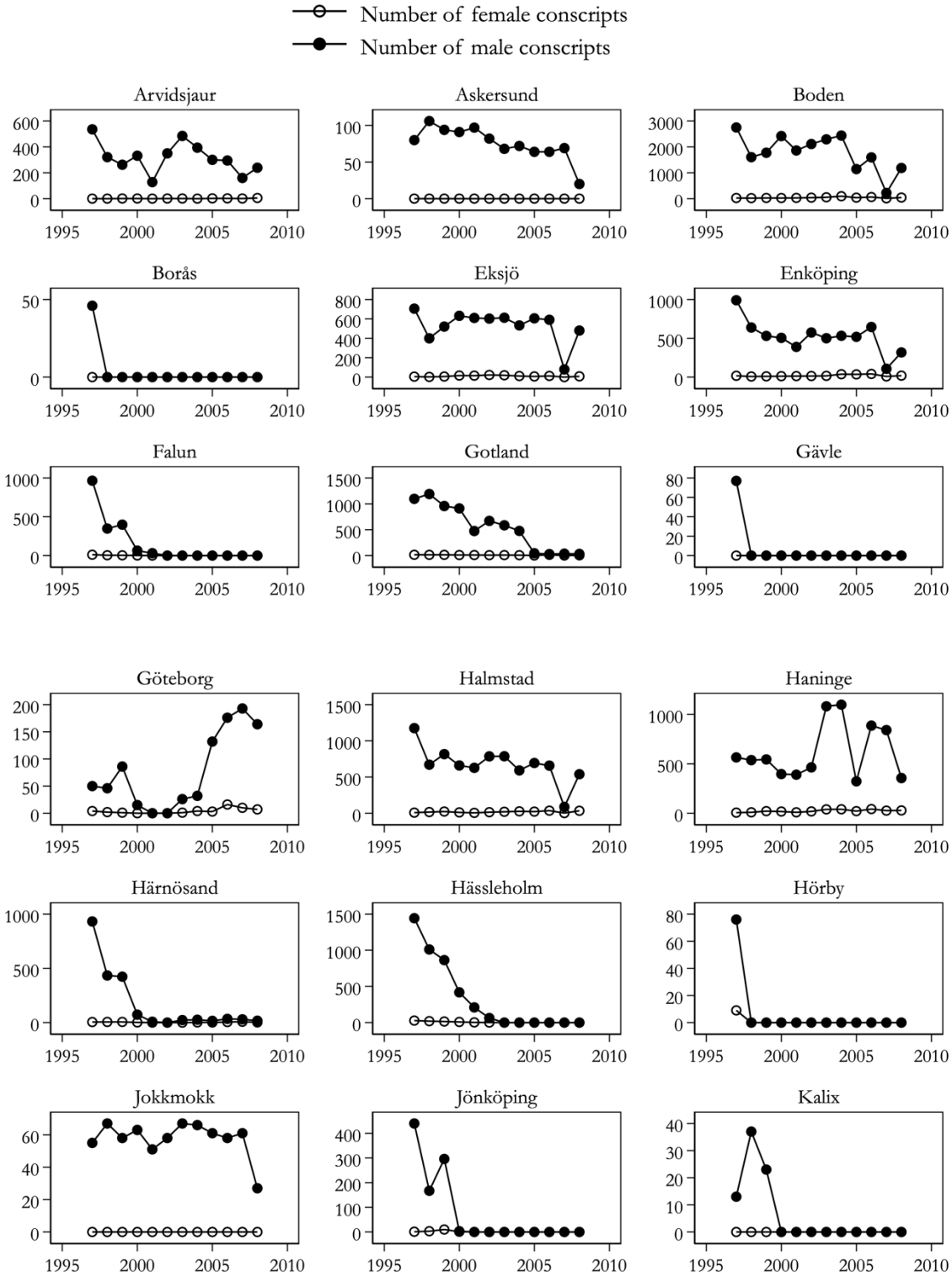
Weiss, K. G. (2010). Too ashamed to report: Deconstructing the shame of sexual victimization. *Feminist Criminology, 5*(3), 286-310. doi:10.1177/1557085110376343

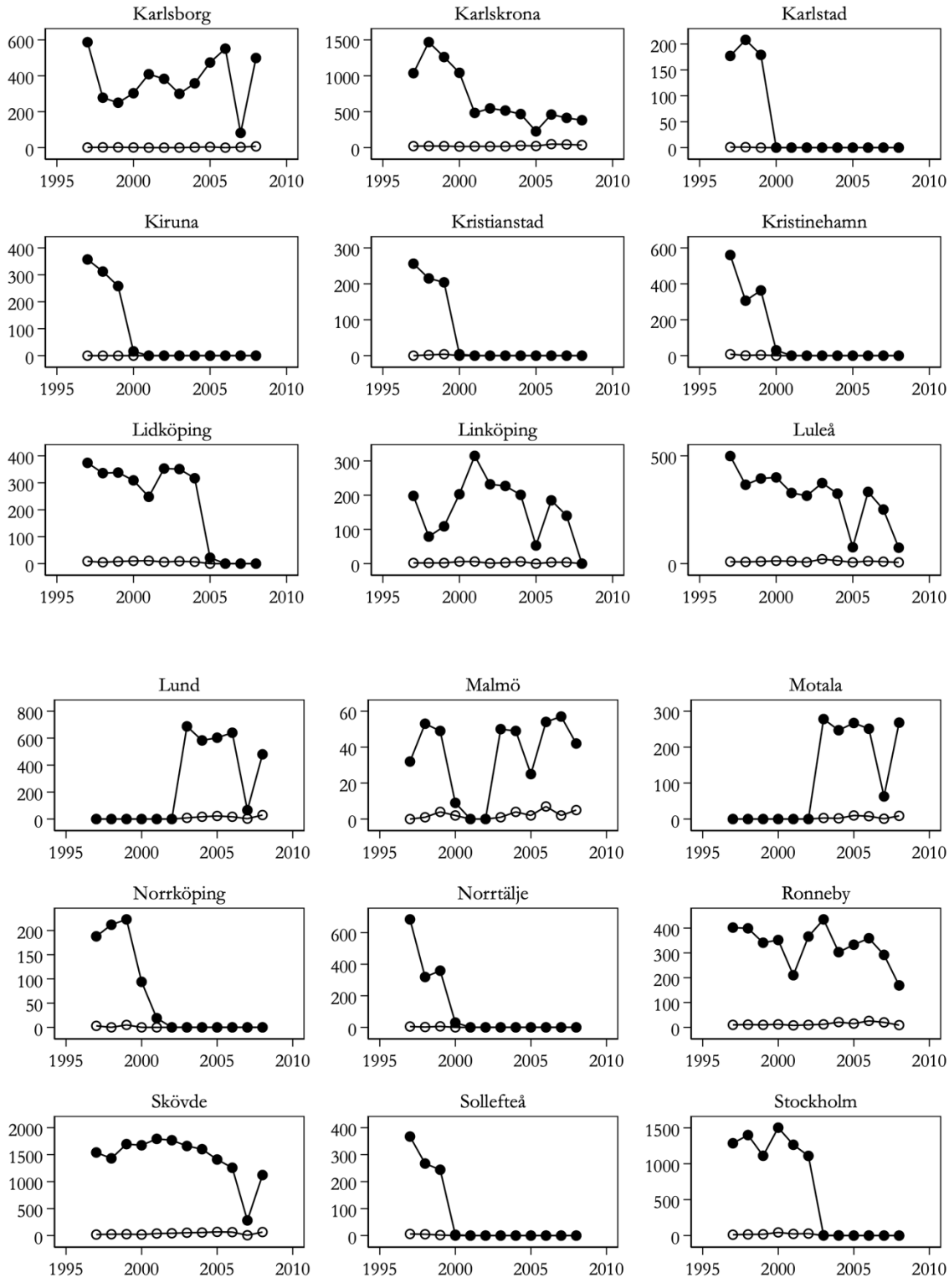
Wooldridge, J. M. (2016). *Introductory econometrics : A modern approach*. Boston, MA: Cengage.

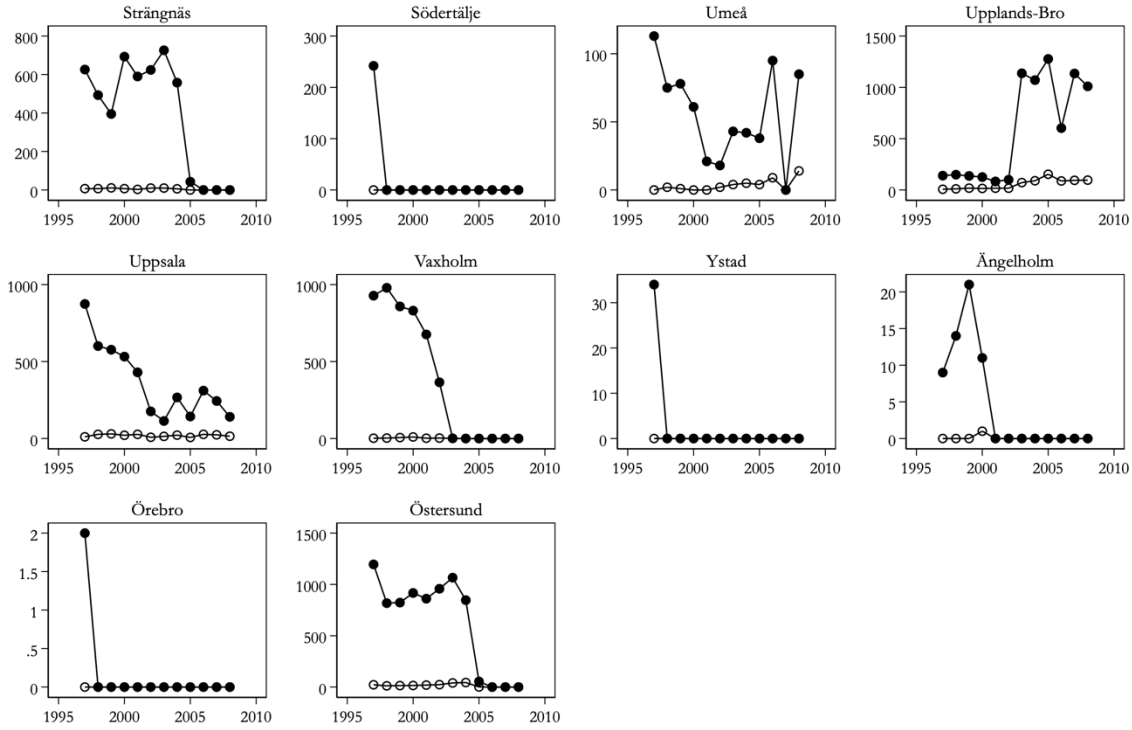
10 Appendix

10.1 Conscripts overview

Figure 6: Number of Conscripts in Each Municipality




Karlsborg
Karlskrona
Karlstad
Kiruna
Kristianstad
Kristinehamn
Lidköping
Linköping
Luleå
Lund
Malmö
Motala
Norrköping
Norrtälje
Ronneby
Skövde
Sollefteå
Stockholm



Note: Source, The Swedish Defence Conscription and Assessment Agency 2021

10.2 Statistical tests

Table 3: F-test for Time Fixed Effects

	Assault	Narcotic Offences	Homicides	Sexual Offences	Appropriative Offences	Sum
Prob > F	0.0495	0.0160	0.0357	0.1786	0.0046	0.0039

Note: H0: time fixed effects = 0.

Table 4: Hausman Test

	Assault	Narcotic Offences	Homicides	Sexual Offences	Appropriative Offences	Sum
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Note: H0: The idiosyncratic errors are not correlated with the regressors.

Table 5: Wooldridge test for autocorrelation

	Assault	Narcotic Offences	Homicides	Sexual Offences	Appropriative Offences	Sum
Prob > chi2	0.0000	0.0000	0.1542	0.3979	0.0000	0.0000

Note: H0: No serial correlation.

Table 6: Wald Test for Groupwise Heteroskedasticity

	Assault	Narcotic Offences	Homicides	Sexual Offences	Appropriative Offences	Sum
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Note: H0: Homoscedasticity.

Table 7: Correlation Matrix for Controlling Variables

	Male conscripts	Unemployment rate	Real income	Population	Share of foreigners	Share of young men
Male conscripts	1					
Unemployment rate	0.0069	1				
Real income	-0.0514	-0.5803	1			
Population	0.0019	-0.031	0.306	1		
Share of foreigners	-0.1059	-0.2203	0.3898	0.5794	1	
Share of young men	-0.1806	-0.5286	0.5201	0.0107	0.1569	1

10.3 Regression versions

Table 8: Summary of Pooled OLS Regression Results for All Municipalities

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	0.0834*	-0.0120	0.000451	0.00133	0.721	0.794
	(0.0379)	(0.102)	(0.000809)	(0.00919)	(0.567)	(0.474)
Unemployment rate	8.379	14.25	0.0434	2.016	197.8	222.5
	(15.88)	(17.96)	(0.342)	(2.312)	(159.9)	(175.2)
Real income	7.267	5.779	0.0965	0.362	-39.14	-25.64
	(6.482)	(7.713)	(0.0973)	(0.757)	(48.27)	(43.73)
Population	0.0114***	0.00892***	0.000151***	0.00147***	0.111***	0.133***
	(0.000864)	(0.000780)	(0.00000596)	(0.0000729)	(0.00199)	(0.00239)
Share of foreigners	-3.379	-4.060	-0.0280	0.889	47.51	40.93
	(8.597)	(7.839)	(0.195)	(1.116)	(48.52)	(54.89)
Share of young men	-196.9***	-145.0**	-3.884***	-19.94**	-1533.4**	-1899.1***
	(41.45)	(53.32)	(0.594)	(5.820)	(448.0)	(480.1)
N	552	552	552	552	552	552
adj. R^2	0.970	0.885	0.925	0.956	0.974	0.983

Notes: The units for unemployment, share of foreigners and share for young men are percentage points and should not be confused as semi-elastic elements. Real income is denoted in thousands SEK. Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Summary of First Difference Regression Results for All Municipalities

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.0193	-0.0461	-0.00118	-0.0127	0.156	0.0766
	(0.0293)	(0.0358)	(0.000873)	(0.0115)	(0.198)	(0.143)
Unemployment rate	10.17	13.81	0.206	-0.463	-59.68	-35.97
	(6.377)	(24.46)	(0.478)	(3.840)	(63.96)	(67.21)
Real income	-15.78	-20.80	-0.0141	-7.264	-56.08	-99.94
	(9.355)	(16.63)	(0.396)	(4.942)	(86.40)	(109.0)
Population	0.0229***	0.0505***	0.000163	0.00476***	-0.214***	-0.136***
	(0.00489)	(0.0116)	(0.000283)	(0.00129)	(0.0326)	(0.0209)
Share of foreigners	26.61**	69.54*	1.145	2.557	-175.4	-75.60
	(10.06)	(32.15)	(0.614)	(10.01)	(131.9)	(120.2)
Share of young men	8.575	-34.32	-0.784	-4.066	170.9	140.4
	(12.96)	(44.91)	(0.707)	(6.057)	(118.6)	(133.3)
N	506	506	506	506	506	506
adj. R^2						
Number of groups	46	46	46	46	46	46

Notes: The units for male unemployment and income are percentage points and thousands SEK respectively. Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Summary of Regression Results for All Municipalities Without Controlling Variables

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.129	-0.3	-0.00342	-0.0306	0.635	0.171
	(-0.158)	(-0.357)	(-0.00248)	(-0.0354)	(-1.098)	(-0.566)
N	552	552	552	552	552	552
adj. R^2	0.275	0.21	0.02	0.177	0.19	0.105
Number of groups	46	46	46	46	46	46

Note: No significant effect is found without controlling variables. Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Summary of Fixed Effect Regression Results for 1% conscript-to-population ratio

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.0262	-0.0742*	-0.0000399	-0.00500	0.0389	-0.0665
	(0.0168)	(0.0297)	(0.000394)	(0.00542)	(0.107)	(0.115)
Unemployment rate	-11.93	-16.76	0.339	-1.369	6.035	-23.68
	(13.91)	(18.11)	(0.257)	(2.757)	(64.85)	(54.60)
Real income	-14.88**	-16.76	-0.0625	-1.334	97.11**	64.07*
	(4.009)	(8.898)	(0.139)	(1.229)	(31.97)	(26.25)
Population	0.0380	0.0447*	0.000199	0.00342	-0.109*	-0.0223
	(0.0190)	(0.0205)	(0.000293)	(0.00308)	(0.0507)	(0.0361)
Share of foreigners	9.107	24.82	0.118	2.207	210.6	246.9**
	(15.30)	(27.71)	(0.308)	(3.556)	(101.5)	(79.50)
Share of young men	2.314	-12.86	0.319	2.536	-102.4	-110.1
	(17.55)	(34.53)	(0.401)	(2.884)	(112.3)	(94.01)
N	252	252	252	252	252	252
adj. R^2	0.623	0.457	0.008	0.164	0.440	0.195
Number of groups	21	21	21	21	21	21

Note: Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Summary of Fixed Effect Regression Results for 2% conscript-to-population ratio

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.00952	-0.0409	0.000304	-0.000609	0.0304	-0.0203
	(0.0225)	(0.0371)	(0.000336)	(0.00287)	(0.0987)	(0.0839)
Unemployment rate	19.26	-2.535	0.557	4.363*	21.35	43.00
	(10.05)	(17.84)	(0.531)	(1.743)	(100.5)	(83.99)
Real income	-14.71*	-12.49	0.139	-1.883**	68.52*	39.57
	(4.834)	(9.002)	(0.122)	(0.548)	(26.27)	(29.81)
Population	0.000947	0.0404	-0.000687	0.000417	-0.133	-0.0920
	(0.00993)	(0.0189)	(0.000521)	(0.00122)	(0.0693)	(0.0750)
Share of foreigners	-1.893	75.90	0.0892	-0.792	39.68	113.0
	(16.98)	(37.56)	(0.471)	(1.671)	(89.68)	(101.9)
Share of young men	-23.63	-68.54	0.852	2.475	4.574	-84.27
	(15.50)	(31.80)	(0.401)	(3.225)	(110.7)	(81.85)
N	144	144	144	144	144	144
adj. R^2	0.612	0.370	0.027	0.291	0.427	0.174
Number of groups	12	12	12	12	12	12

Note: Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Summary of Fixed Effect Regression Results for 3% conscript-to-population ratio

	(1) Assault	(2) Narcotic Offences	(3) Homicides	(4) Sexual Offences	(5) Appropriative Offences	(6) Sum
Male conscripts	-0.0158	-0.0461	0.000235	-0.00195	0.197*	0.133
	(0.0220)	(0.0377)	(0.000454)	(0.00311)	(0.0655)	(0.0709)
Unemployment rate	28.93**	-6.918	-0.0285	4.288*	-60.98	-34.71
	(7.302)	(22.49)	(0.440)	(1.375)	(67.51)	(53.15)
Real income	-12.48*	-4.212	-0.103	-2.412	12.90	-6.300
	(4.193)	(6.569)	(0.0986)	(1.202)	(31.49)	(32.15)
Population	-0.0204*	0.0209	0.000430*	0.00522**	0.0960	0.102
	(0.00752)	(0.0277)	(0.000162)	(0.00138)	(0.0646)	(0.0508)
Share of foreigners	8.280	44.16	-0.225	3.361	-57.38	-1.810
	(13.90)	(26.78)	(0.172)	(3.073)	(106.6)	(107.8)
Share of young men	-30.41	-56.21	0.505	-2.466	23.44	-65.14
	(22.15)	(26.38)	(0.232)	(3.354)	(59.08)	(64.07)
N	84	84	84	84	84	84
adj. R^2	0.688	0.151	-0.064	0.346	0.512	0.173
Number of groups	7	7	7	7	7	7

Note: Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$